General comments

Part I of “Unshielded precipitation gauge collection efficiency with wind speed and hydrometeor fall velocity” describes a modelling experiment designed to estimate precipitation undercatch in an unshielded precipitation gauge. The work focuses on the use of hydrometeor fall velocity to create improved transfer functions available to adjust unshielded precipitation measurements. The background and importance of the problem are well described in the introduction, which provides an excellent overview of past work in the modeling of precipitation undercatch. The methods and results are well documented, and the manuscript is generally very well written and easy to follow. The topic of undercatch is an important one, and this work is both new and useful, as it addresses the most difficult outstanding questions in precipitation undercatch; the manuscript establishes a valid way to reduce the significant uncertainty that precipitation transfer functions suffer from, and future work may also prove that this new approach can help reduce the site-to-site variability of collection efficiency and the resultant biases and uncertainty.

There are a couple of methodological points which need to be explored or explained more fully. These are described in more detail in the specific comments below, but I find the unrealistic background surface layer atmospheric flow problematic. In addition, the concept of a wind speed threshold above which collection efficiency is equal to zero is both impractical, and in my opinion theoretically unsound. However, I am not proposing that the entire model be redesigned, as it is certainly a valuable study as-is, especially as demonstrated by the accompanying Part II of this manuscript. I would however like to see these shortcomings handled differently within the manuscript.

After completing my review, I read the reviews from Referees #1 and #2, and feel compelled to write that I disagree with their main point, which is that these manuscripts are not novel enough to merit publication. I am ambivalent about whether or not they need to be published as two separate papers; I will leave that up to the editor. However, I maintain that the main point of this work, which is the inclusion of the fall velocity in a transfer function, is indeed both new and useful.

Theriault et al. (2012) includes a transfer function with a snowflake type parameter in it, but not the hydrometeor fall velocity. While Theriault et al. (2012) helped demonstrate the connection between hydrometeor fall speed and catch efficiency, and in general the importance of snowflake type, it did not include an easily applicable method for the improvement of operational precipitation measurements. While crystal type and hydrometeor fall velocity are certainly linked, as both manuscripts demonstrate, the use of the hydrometeor fall velocity, which can be measured relatively reliably and automatically, is important as a characteristic separate from the crystal type. All hydrometeors (not just snowflakes) have a measurable fall velocity, and as demonstrated by the present manuscripts under review, this fall velocity can be used to improve the collection efficiency transfer function. This is new. None of the references offered by Reviewer #1 and Reviewer #2 demonstrate a transfer function that includes the hydrometeor fall velocity. Nor for that matter, in my opinion, do any of those papers offer practical improvements to the currently available transfer functions that can be applied in an operational network. It is also worth noting that most of the important papers that Reviewer #1 and Reviewer #2 cite as evidence of the lack of novelty in the present paper were already cited in the present paper; it is not as if the authors of the paper under review were hiding the fact that this past work existed, or that it influenced their own work.

It is also worth noting that the use of the fall velocity is very different from the use of precipitation intensity for the improvement of collection efficiency transfer functions. While there may be some general correlation between precipitation intensity and hydrometeor type, precipitation intensity is not a good proxy for hydrometeor type, and in fact has real limitations for use in collection efficiency transfer functions. One of the most significant of these limitations is the fact that both precipitation intensity and collection efficiency are heavily dependent on the same precipitation measurement; they are not independent variables, and in such a case it is easy to demonstrate correlations that have no real or physical relevance.

Specific comments

Ln. 53. Explain what is meant by, "a sharper decay and higher intercept of a negative exponential distribution." The decay is with respect to what? This actually does bring to mind an altered curve, although I'm not sure if I am seeing it correctly. Anyway, I wouldn't write something like this and expect my readers to be able to understand it. In addition, I have no idea what are on the x- and y- axes of this imagined curve.

Ln. 147. Why was the ground modeled as a frictionless wall? I am afraid I may be climbing up onto the soapbox here. However, I maintain that is not a 'get off my lawn' comment, because modeling atmospheric flow is not really my specialty. I know others have modeled gauge catch efficiency using the same boundary condition. But it results in an unrealistic vertical wind speed profile, in which the horizontal wind does not decrease with height, and is not zero at the ground. Just because others have done it, does not mean it makes sense. Especially when modeling a large shield (which is admittedly not the case here), a realistic vertical wind speed profile is needed to simulate realistic flow over the shield. But more importantly, without a zero-slip boundary condition at the surface, the model will not generate realistic background turbulence; in neutral atmospheric conditions, turbulence near the surface is generated by wind shear. With a frictionless surface there will presumably be no wind shear, and also no background turbulence. To clarify, I am not talking about the turbulence created by the gauge, but by the surface of the earth. This 'normal' background surface layer turbulence is important because it affects the flow over the gauge and the hydrometeors falling towards the gauge. In real life, the atmospheric flow at the earth's surface is not laminar. The assumption that undercatch can be modeled accurately in laminar background atmospheric flow should at least be discussed, along with the possible shortcomings.

Table 1. u_w hasn't been defined yet. Or if it has, I can't find it. Also, I find this a confusing choice as the symbol for the free stream wind speed. This is because w is often used for the vertical wind speed, and because u_x, u_y, and u_z are also used to describe different components of the wind velocity; u_w looks to me like another way to describe the vertical wind speed.

Ln. 198. Based on the statement that hydrometeor interactions were ignored (ln. 188), I am guessing that "interactions *within* the gauge orifice" should be changed to, "interactions *with* the gauge orifice."

Ln. 285. The way this is currently written it could be misinterpreted to mean that u* is the free-stream wind speed, not the, "peak velocity along the gauge centerline normalized by the free-stream wind speed." Perhaps the normalization could be moved to the end of the sentence – this sort of normalization is to be expected anyway, so I would argue that it isn't a critical part of the definition. "Peak velocities along the gauge centerline (u*) are compared… in Fig. 3, with the centerline velocities normalized by the free-stream wind speed." Maybe? Also, I find u* a confusing choice, as ustar (u_*) is an often-used variable with a completely different and well-established usage.

Figure 3. I believe the y-axis should be labeled u*, not z*. Also include u_w (or its replacement!) in the caption in parenthesis after, "normalized free-stream velocity" to help clarify the meaning of the panel (a) and (b) titles.

Figure 4. This is an excellent figure. I suspect we will see it reference and recycled many times, in future presentations.

Figure 5. Small issue, but the legend shows open yellow squares for ice pellets, and the plot shows closed yellow squares (u_f = 5 m s^{-1}).

Ln. 320. Clarify by changing "hydrometeors up to about 3 m s^{-1} wind speed" to, "hydrometeors for horizontal wind speeds up to about 3 m s^{-1}". I was confused by all the different speeds in this sentence.

Ln. 311 – 324. Some explanation of why the "dry snow" results are so unrealistic is needed. Experimental collection efficiencies are never this low (or zero). Is your hypothesis that this is because pure "dry snow" rarely occurs? Or is it because the experimental collection curves are derived wrong? I will say more about this elsewhere, but I find the suggestion that collection efficiency drops to zero problematic (and impractical). I suspect that it may be due to the fact that the modeled background flow is not turbulent. In the real world, surface layer flow and particle dispersion are stochastic processes. Given enough time or water, some hydrometeors will always be forced into the gauge by an errant eddy, no matter how slowly they fall or how high the wind speed is. The trajectories in Figure 4 are fine for what they are, but they show how hydrometeors behave in a laminar wind tunnel, not in actual turbulent surface layer flow. Turbulence intensity typically increases faster than the mean wind speed near the land surface, so it actually becomes more important as the wind speed increases. This may be why most experimental results reveal a sigmoid or exponential response of collection efficiency to wind speed, with the sensitivity of collection efficiency to increasing wind speed decreased (with the sigmoid function becoming flat, or unchanging with respect to wind speed) at high wind speeds.

Ln 335, Eq. 18. Would it be possible to derive a collection efficiency equation, or its functional form, from the equations used within the model? I am a little disappointed that a modeling paper relies on an empirical equation.

Ln. 344 – 345. I am again flummoxed by this concept that collection efficiency = zero at some point. What purpose does it serve? Is there any measurement evidence to support it? And how does one correct a precipitation even that occurs when the collection efficiency is defined as zero? I believe that the introduction of this zero-collection-efficiency concept and the emphasis placed on it in this paper may confuse others and hinder future progress in collection efficiency research. I grant that at low temperatures and high winds, an unshielded gauge can fail to measure any precipitation, but that is in part because most 30-min snowfall ‘events’ are near the measurement threshold of the gauge, in the 0 – 0.4 mm range. But just because we can’t always measure it, doesn’t mean it is zero. And if collection efficiency is defined as zero by the transfer function, how to we apply this function when precipitation is measured under these conditions. In a large enough dataset, we will be very hard pressed to find any commonly-occurring environmental conditions under which the reference catches precipitation and the unshielded gauge NEVER catches precipitation. But this is indeed what this theory prescribes, that there are certain conditions under which it is impossible for an unshielded gauge to collect certain hydrometeor types. That is very tall claim. The existence of such conditions in the real world should be demonstrated before making zero collection efficiency a central part of the theory. At a minimum, the discrepancies between past experimental results and the modeled results should be discussed.

Figure 6 and ln 349 – ln. 352. If I understand correctly, these results were produced using Equation 9 and the fall velocity, not the more complex precipitation characteristics. So why was only wet snow shown (or discussed) at u_f = 1.5 m s^{-1}? In theory, the same transfer function would be used for different precipitation types, given the same fall velocity. But not all the precipitation types are shown or discussed. Why aren’t all the collection efficiency curves shown in Figure 5 shown here? Was the figure too busy? In all honestly, initially I was confused, and thought that only wet snow was modeled at u_f = 1.5 m s^{-1}, but I believe I understand now that these results should be equally valid for all precipitation types, as they are purely a function of fall velocity.

Ln. 389. Clarify that the dependence of collection efficiency on hydrometeor type and precipitation intensity was modeled solely based on differences in hydrometeor fall velocity.

Figure 9 and its discussion. Explain why none these curves look like the ‘dry snow’ curves in Figure 6. I believe it is because of the distribution of different hydrometeor sizes (and fall velocities), but it is still worth pointing out.

Ln. 507. Delete “with” in, “results with over…”

Ln. 515. Rephrase to clarify that 1.0 m s^{-1} refers to the fall velocity.

Ln. 525. Delete, “considered to be.”

Ln. 535. Delete, “that is.”

Ln. 573 – 577. Interesting. I had no idea.

Ln. 588. The phrase, “have reduced ability to be collected” is awkward as written.

]Ln. 613, 614, 615, 619, 620, 624. I find the use of “overall” confusing. It has too many other common meanings. For example, my first read of, “Overall collection efficiencies with precipitation intensity…” on ln. 613 made me think that a comma after “overall,” had been omitted. Looking back, I see that the term “overall” is nicely defined in Section 2.3, and again on ln. 370, but the use of a term that is less commonly used in normal English would make it clearer that it has a specific meaning. Perhaps, “integrated catch efficiency?”

Ln. 624, Clarify that, “conditions when solid, liquid, or mixed precipitation can be present” refers to conditions when all of these types may be occurring, such as near-zero degrees C. As-is, 30 deg C in a thunderstorm qualifies as a time when, “solid, liquid, or mixed precipitation can be present,” as does very cold conditions, when only solid precipitation can occur. I am sure there are better ways to write it, but one suggestion that remains fairly close to what is written is, “conditions when solid, liquid, *and* mixed precipitation can *all* be present.” Or, “conditions when it is difficult to know the phase of the precipitation, “or near-zero degrees…”

Ln. 644 – 645. In my opinion the sentence beginning with, “The results from the ability of the hydrometeor…” can be removed. It is redundant; the previous sentence makes this point.